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ABSTRACT

Reports on the AMS Conference on Communication Problems in the Mathematical Sciences which was organized by the Society's Committee to Monitor Problems in Communication is presented. The scientific program of the conference included a number of special addresses and five panel discussions. Among the items discussed were (1) the need for founding a new American survey journal at the research level along the lines of the Russian journal, USPEHI, (2) the Information Exchange Groups established in the field of biochemistry to facilitate preprint exchange, (3) a description of the organization and activities of the American Institute of Physics, (4) the distinction between "important" papers and technical papers, (5) several types of editorial problems, (6) duplication of results, (7) the merits of reviews versus author abstracts, and (8) the history of several reviewing journals from various countries. (RP)

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FINAL REPORT

CONFERENCE ON COMMUNICATION PROBLEMS IN THE MATHEMATICAL SCIENCES

December 5-7, 1967

U.S. DEPARTMENT OF HEALTH, EDUCATION & WELFARE
OFFICE OF EDUCATION

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December 5-7, 1967

The Conference on Communication Problems in the Mathematical Sciences was supported by the National Science Foundation under NSF grant GN-702.

The AMS Conference on Communication Problems in the Mathematical Sciences, organized by the Society's Committee to Monitor Problems in Communication, was held in the Sheraton-Biltmore Hotel in Providence, Rhode Island on December 5-7, 1967. The scientific program of the conference included a number of special addresses and five panel discussions.

The conference opened with a welcoming address by Daniel Zelinsky, Northwestern University, chairman of the Society's original Committee on Information Exchange and Publication in Mathematics and a member of the present Committee to Monitor Problems in Communication.

Professor Zelinsky stated that the question of communication is vital to the AMS and the problems are shared by all mathematical research organizations. Therefore, all such organizations should benefit from the conference. Organizations representing disciplines outside mathematics can provide an indication of how mathematics can serve them and the border areas.

He proposed that the conferees work from the position that mathematics is at a serious but not yet critical stage in the information avalanche. Since the discipline is not yet caught up in an emergency situation, where solutions must be found for small, interrelated problems that should have been solved previously, mathematicians are free to consider issues in the large. The present state of communication in the field allows for the expression and consideration of radical ideas.

For example, do we really need primary journals? Important communication actually goes on before publication. Perhaps there should be an increased emphasis on informal communication, and journals should be replaced by expository writing at a high level.

Also, the present meeting structure may be outdated. There are rapidly becoming too many mathematicians at meetings, and it is almost impossible for one mathematician to have meaningful contact with an appreciable number of other mathematicians. Why have large meetings? Instead, travel money could be expended on specific visits of specialists; large, cross-disciplinary meetings could be held once every two years or so.

Professor Zelinsky suggested that these and other wild-eyed schemes would be advanced at the conference, and that the participants should consider these seriously.

He went on to say that the present drive toward thinking about communication problems stems from recent technical developments; i.e., computers and various new reproduction techniques. Publication, storage, distribution, etc. can be mechanized and automated. Mathematics should capitalize on these improvements in method. But, we should remember that we are not bound by these improvements. Our responsibility at this time is to be selective, to decide which activities can profitably be mechanized and which should remain in the hands of specialized personnel.

Professor Zelinsky then discussed the format of the conference. He noted that the proceedings would hopefully serve as a stimulus to the whole mathematical community to become conscious of the problems of communication in mathematics and to think about effective means of meeting these problems.

After Professor Zelinsky's address, the Panel on Exposition and Book Publishing was convened. The Moderator, Professor Edwin Hewitt, University of Washington, introduced the first speaker, Professor Felix Browder, University of Chicago. Professor Browder discussed the need for founding a new American survey journal at the research level along the lines of the Russian journal, USPEHI.

Both Professor Norman Steenrod, Princeton University, and Professor Hans Freudenthal, University of Utrecht, The Netherlands, discussed types and methods of exposition.

Professor Steenrod stated that definitions of the objectives of the various types of publications are needed before any general improvement in the quality of exposition can be made. He presented his view of what a research article is and what appropriate exposition means for that kind of article.

He regards the primary purpose of a research article as the communication of new ideas and new forms of old ideas to experts in the immediate field so that they can use them, modify them, and create new approaches to them. The author presents an idea and what it can do from his point of view, and his readers rearrange it, formulate it differently, and perhaps create new uses for it beyond those seen by the author. Professor Steenrod does not see the purpose of the research article as being the presentation or reporting of new results with emphasis on the results.

What is meant by good exposition of this type of publication? It is clear that good exposition is intended to disseminate new ideas as quickly and as sharply as possible so that the audience can seize on the ideas and not have to wade through a mass of material that the expert already knows in order to find the good ideas. Any lengthy discussion of known results only serves to obscure what the expert is trying to find. Also, the paper, if a good one, will quickly be replaced by other papers, written on the same idea, which are improvements. The paper has a temporary life--the better the paper, the shorter the life. Why then should the paper contain large amounts of exposition if its life is so short?

A well-written research article is not usually readable by or accessible to mathematicians in other fields, and this should not be a disturbing fact. If a mathematician wants to read what is going on in some other area, why should he waste his time trying to read about an idea that is in an early, unfinished state? He should instead go back and read some expository work on the idea which describes it from its development a number of years ago to its present shape.

Professor Steenrod noted that there are no books available on how to write books on mathematics. There is no accumulation of knowledge on how to do the job. The author must find out what the problems are himself, using books that others have written as a guide. He must find his own solutions to the problems of structure, organization, and exposition.

Professor Steenrod proposed that the AMS appoint a committee to prepare a pamphlet on how to write mathematical books, at the research or graduate text level. If the members of the committee cannot come to an agreement on what is meant by good exposition, each one could point out mistakes he thinks others have made.

He also suggested that the AMS appoint a committee to determine criteria for acceptance of papers in each AMS publication.

Professor Freudenthal suggested that an inquiry into the learning and reading habits of creative mathematicians would be useful as a preliminary step in improving mathematical writing and publication. He described his own system of learning by guided reinvention and compared it to the other common method of following a given exposition. Professor Freudenthal's method involves proceeding from global to local understanding; he said that most mathematical work is written for the mathematician who proceeds in the opposite way, namely, from local to global understanding, and he suggested that more work should be directed to those who employ the first method. He also suggested that authors attempt to describe how they would have liked to discover their results; that is, the steps they would

have taken had they proceeded in the best possible way. This would help people who learn globally and would also aid guided reinvention.

Professor Freudenthal then discussed the desirability of a systematic encyclopedia, an alphabetic encyclopedia, and monographs and surveys of three distinct types--for those who are unfamiliar with the subject and do not want a profound, comprehensive introduction; for those who are unfamiliar with the field and do want a comprehensive treatment; and for those who understand all details of the subject and want a very thorough, systematic treatment.

Finally, he mentioned improving the formal appearance of publications.

Professor C. B. Morrey, Jr., President of the Society, addressed the participants at lunch. His address included some historical notes and emphasized the increasing importance of communication problems.

The second group was the Panel on Primary Journals and Related Information Systems, which was broken down into two sections. The first section, moderated by Professor Alex Rosenberg, convened in the afternoon of December 5. The first speaker was Dr. Norman Cottrel, Office of State Technical Service Program, formerly of the American Society of Metals. Dr. Cottrel, who was the former Director of Documentation for the ASM, talked about that society's information system program.

He first summarized the background of ASM. Its information program represented one of the pioneering projects in the information technology field. The ASM decided to computerize the world's technical literature in metallurgy, the purpose being to provide the most effective dissemination, storage, and retrieval of the information in the field.

Dr. Cottrel described some of the problems encountered by the ASM in implementing and operating its system. First, the society assumed intuitively that the size of the potential audience for the system would be equal to the size of the ASM membership. This proved to be false. It was discovered that the prime value of the service was to serve researchers in fields other than their area of principal competence. The men at the top of their fields did not need help in acquiring information in those fields.

Also, the system lacked an effective feedback mechanism. There was subscriber criticism, but there was no way to put it back in the system. Therefore, the ASM built a feedback loop between the system and the system users. Actual user conferences were organized, where users met and discussed problems. Dr. Cottrel stressed the critical importance of establishing a feedback mechanism within any system the mathematical organizations might design.

In the ASM system, revenue could not keep up with costs. As the quality of service improved, the number of users grew faster than the system could take care of them. ASM found that it had not predicted its costs accurately, and it made an attempt to become self-supporting prematurely. Therefore, in the nine months preceding the conference, ASM worked to decentralize the system. Dr. Cottrel stated that, providing this effort is successful, a valuable service to users will continue.

The second speaker was Dr. Robert Gordon, Secretary of the Council of Biology Editors. He described the Information Exchange Groups (IEG's) established in the field of biochemistry to facilitate preprint exchange.

The IEG experiment began with a discussion among two leaders in the field and a staff member of the National Institutes of Health about information exchange in biochemistry.

The discussion centered on three points: The problems of unplanned duplication of basic research; the advantages of rapid exchange of information enjoyed by the invisible college, resulting in rapid resolution of research problems; and the problem of isolation of invisible college groups lacking an historical connection. The NIH set up an operational study of rapid exchange of research information between groups of scientists working in specific problem areas. The study was billed as an experiment, but it had no control element built in and it lacked design. (Actually the program represented a type of user study.) The effectiveness of the IEG groups was evaluated by a questionnaire which was distributed to participants as an afterthought.

The stated premise for development of IEG's was as follows: Given maximum current information in a rigidly defined problem area, more intelligent day-by-day research decisions can be made by the active researcher, thus accelerating the resolution of the specific problem or problem complex. Once the subject problem is resolved, the specific IEG disbands. It was expected that all problems relating to the use of the referee system would be reduced on the premise that each researcher is capable of acting as his own referee.

The role of the NIH was that of financier, printer, and distributor. Material was reproduced by photo offset and distributed by surface mail to domestic scientists and air mailed to foreign scientists. Each item was called an IEG memorandum and given a sequential number in order of its receipt at NIH. Each IEG group operated more or less independently, but the role of the NIH as management did increase in time.

One ground rule, common to all IEG groups and agreed to by each participant, was that the memoranda would be treated as personal communication and that no mention could be made of them in the formal literature except as personal communications; that each member always assumed the responsibility for providing due credit for information obtained via the IEG memoranda on matters where priority might arise. There was no censorship or refereeing at all.

In the course of six years, seven IEG groups were formed. A total of 2,561 memoranda were distributed, produced by 3,660 participants. Of the total of 2,561 memoranda produced, 87 took the form of preprints; i.e., duplicated manuscripts that had been or were about to be submitted for publication in the form of literature. Most of the items submitted were full manuscripts rather than short preliminary reports, etc. Since almost no one would risk presenting an inferior article or short potboiler to an audience representing the cutting edge of science, trash did not materialize in the majority of IEG groups. There were few memoranda which openly criticized a previously distributed communication; however, there was some evidence of private communication whose amount is unknown. Therefore, it is not known whether the establishment of IEG's did in fact result in increased polemics.

In the area of rapid transfer, participants in the IEG system received material at least six months earlier than they would have received it through conventional channels, i.e., publication. For the IEG group which was established earliest, it was estimated that participants got 90% of the major papers which would be published in their given field.

At the end of the experiment, the idea of establishing an IEG around a rigidly defined problem area was identified as the cardinal characteristic for the success of the group. If a group embraced too large a problem concept, unwanted material would appear in the communication quite early, and there would be no cohesiveness to the group.

Of 466 respondents to the questionnaire submitted to participants at the conclusion of the experiment, 346 reported that information received through IEG memoranda prevented unnecessary duplication of research.

Concerning criteria for membership in IEG's, participants were selected by the chairman of each IEG. Initially the groups included only the invisible college, the in-group, but through time the concept of a corps consisting of cutting-edge scientists surrounded by young, emerging colleagues, appeared. There was a six-fold increase in membership by the termination of the experiment. The membership of all seven groups was international.

It was estimated from the data received from participants that a total of 1,969 research months, or 163 years, was saved by the use of IEG memoranda.

There was some controversy about the relationship between IEG memoranda and formal publication. It was suggested that preprint circulation is an established practice which is acceptable if the number of copies circulated is fairly small (indicating that the circulation of IEG's made them unacceptable.) The question would seem to be, At what number does the preprint assume the status of formal publication? Another group of critics expressed their view that the real danger of the IEG system is that the system will reduce the usefulness of existing journals and may ultimately supercede them. Another criticism was that preprints infringe on copyright, which suggests that there is an economic fear attached to the appearance of preprints.

In Dr. Gordon's view, the basic question posed by the IEG phenomena was, "What constitutes publication?"

The third speaker was Dr. Hugh Wolfe, of the American Institute of Physics. Dr. Wolfe first described the organization and activities of AIP. The American Institute of Physics is a federation of seven professional societies, the largest of which is the American Physical Society. AIP has an unduplicated membership of 45,000. Its activities include publication, public relations, educational programs, and operation of a center for the history and philosophy of physics. It publishes sixteen primary journals, for which it makes heavy use of publication charges, and it translates thirteen Russian journals on a cover-to-cover basis. The latter are self-supporting. AIP's output represents one third of the world's journal literature in physics, as covered by PHYSICS ABSTRACTS. The overall rejection rate for AIP journals is 20%. The referee system has a sizeable effect on the quality of the journals; of those papers accepted, a large number are returned to the authors for revision.

In physics, there are a number of "in" journals. Physical Review Letters, Applied Physics Letters and Physics Letters are all intended to give high-speed publication to short, important announcements of new research results. They are all refereed, and the rejection rate is much higher than for regular journals, principally because much of the material submitted is all right but does not have the urgency which demands this kind of publication. These journals are in print within two months of receipt of the papers by the editors. Special techniques, such as having referees telephone their comments to the editors, have been instituted to save time.

AIP accepts no articles for publication unless they are accompanied by author abstracts, and referees are instructed to consider the abstract as well as the paper in deciding whether a paper should be published. When it is in page proof, the first page of every article, including the bibliographic information and the abstract, is sent in advance of publication to the principal abstracting services for physics. PHYSICS ABSTRACTS, in addition to publishing abstracts, puts out a current awareness publication called Current Papers in Physics. The service, which publishes twice a month, uses the major subject headings of PHYSICS ABSTRACTS and lists all of the articles which have come in in the past two weeks by title, author, and bibliographic reference, under these subject headings. References are grouped by journal.

Dr. Wolfe then described AIP's present effort to develop a plan for a national information system in physics. The goal is to integrate the total information program, beginning with primary publication and publication of the abstracting and other secondary services, and finally providing the machinery and indexing information to provide for mechanized information storage and retrieval systems. AIP intends to develop its system with a maximum of international cooperation.

Dr. Wolfe described in some detail two specific aspects of the information system program. In the area of indexing, AIP is developing a classification system using multiple coordinates to locate the particular area of physics in which a paper belongs as a volume element in a multidimensional space. Within that area, further refinement of the indexing can be accomplished through the use of appropriate key words or phrases. AIP is working on mechanizing the publication system by means of computer-driven photocomposition.

The afternoon session concluded with an address by Professor Everett Pitcher, Secretary of the AMS. Professor Pitcher described the Society's information exchange projects--Research on Machine Aids to an Editor of Scientific Translations, a Study on Computer Aids for Tape Control of Photocomposing Machines, and development of an Information Retrieval System for the journal, MATHEMATICS OF COMPUTATION.

The second section of the Panel on Primary Journals and Related Information Systems met on the morning of Wednesday, December 6. Professor Robert Bartle, University of Illinois, was the Moderator. He introduced the first speaker, Dr. J. S. Traub, Bell Laboratories.

Dr. Traub noted that the major vehicle for communicating research results is the primary journal, which provides a system for formal, public and orderly communication. He offered some definitions of these facets of the journal: It is <u>formal</u>, because papers can be cited and retrieved unambiguously. It is <u>public</u>, because journals are available to anyone in libraries or by subscription, and because anyone can submit a paper. It is <u>orderly</u>, because input is accepted or rejected by the community itself, only on the basis of merit. Publication of the journal remains vital to the scientific community.

Dr. Traub pointed out that there are three partners in journal publication: They are readers, authors, and publishers, and all three have their complaints. The reader feels he is the victim of an information explosion, the author feels he is the victim of excessive delay, and the publisher is caught between demands for more and more publication and the economic facts of life.

Thus, Dr. Traub feels that it is now time for the following proposal: that journals stop binding papers into issues and distribute to subscribers a stream of titles, papers, and abstracts according to their own personal (and changing) desires. Under this system it would still be possible to obtain all papers accepted by a journal, and indeed most libraries and some individuals would continue to do so.

He suggests that authors describe the interests of the readers to which the paper should be sent, and that the reader describe the types of papers he wishes to receive. The reader could change his profile. Matching and selection would be accomplished by computer. The vocabulary by which papers and interests would be described would be specific, small, and well structured, in order to be comprehensible as a whole by the mathematician.

Dr. Traub described the Mercury System as an example. (He was one of the creators of the system.) In the Mercury System, the mathematics vocabulary is one page long. It is a tree structure with three levels, containing a total of about 50 terms. By using the three-level vocabulary, an individual can obtain titles, abstracts, or entire reports.

When a new vocabulary is written, cross references are inserted to point back toward the existing vocabulary. Dr. Traub noted that vocabularies are written by researchers in the field, not by information specialists.

He suggested that each professional society could choose its own vocabulary, tailored to its own interests, and participate in a central retrieval service with central billing. An individual subscriber could receive world-wide coverage of the literature through one payment to the central service. Of course, the vocabularies chosen by the various societies would have to be compatible. Since many societies are now contemplating the development of SDI, it is important that they maintain close contact.

The second speaker was Professor Jean Dieudonne, Dean of the University of Nice. ✓ Professor Dieudonne discussed the distinction between "important" papers and technical papers. He stated that, in most journals, the proportion of technical papers to important papers is large. In MATHEMATICAL REVIEWS, about 20% of the papers reviewed may be classified as important papers, while in the ANNALS OF MATHEMATICS or the AMERICAN JOURNAL OF MATHEMATICS, the proportion may go as high as 80% or 90%. In other journals, the proportion is 50% or lower, as in the PROCEEDINGS OF THE AMS, where the largest proportion of papers are technical papers.

The mathematical community needs 1) speedy information on important papers, and 2) information, period, on all papers. Producers of mathematical information must achieve these goals without putting a strain on the time of reviewers and referees.

Most journals have backlogs of twelve to twenty months. It is sometimes two, three, or four years before some important papers are reviewed in MATH REVIEWS. This situation is satisfactory for the inner circle of mathematicians who are in communication with authors of important papers; however, isolated places have no communication with new developments.

One unfortunate result of the present system is that bad or trivial papers are reviewed before good ones. A tremendous amount of material is being published, and the referee and reviewer are flooded with material. However, the referee system is clearly necessary and must stay, and reviews are valuable and cannot be eliminated.

Dieudonne proposes that each author send a comprehensive abstract (like an MR review) with his paper when he submits it for publication. The abstract could be sent to the referee, who would decide whether the paper were important enough to be published in the conventional way. If it were bad or trivial, he would reject it. If it were good, but not important, the referee could recommend that it be prepared in microfilm and stored, and that only its abstract be published.

The manuscript could then be typed by the author on prepared sheets and photographed. It could be stored in a central office operated by MATHEMATICAL REVIEWS, and individuals requesting a copy could obtain it directly from the MATH REVIEWS office.

The referee could approve the abstract and send it to MR for printing. The referee knows more about a paper than a reviewer and has checked its accuracy. If the abstract were not sufficiently informative, the referee could write it himself or send the paper to a third party and ask him to review it.

If this system were put into effect, it would eliminate 90% of the need for reviewers. MATH REVIEWS would become, to a certain extent, an abstract journal, but this would not change the essential character of the publication. MR would be able to publish reviews

much faster since the work would be done by referees before the actual publication of the paper. Only for books or for particularly controversial papers would it be necessary to refer a work to a third individual independent of the referee. This system would aid MR and would free primary journals to publish only important papers.

Professor Dieudonne also considered the problem of how to induce mathematicians to write down important ideas. Some never formally publish their ideas; they just communicate them informally to their friends. They claim their theorems but produce no proofs. Dieudonne suggested publishing an editor's note with papers that quote unproved theorems, stating "The results of this paper depend on the theorem of _____, which no one has seen and which must be considered a conjecture."

At the end of Dieudonne's address, a member of the audience noted that, regarding the proposed system of refereeing and publishing, the anonymity of the referee producing reviews, as opposed to the nonanonymity of present reviewers, raises a problem of principle.

The next speaker was Professor Eldon Dyer, City University of New York. He described several types of editorial problems: how to decide which manuscripts to accept and reject outright; how to select the best possible referees who are willing to check arguments and to contribute suggestions for a manuscript; how to avoid calling on a referee who will be affected by his personal relations with the author; how to avoid calling on one referee too often. He also described the more difficult financial and production problems of journal publication--of reducing time lags and of paying for publications. He put forth a proposal for solving these problems for the PROCEEDINGS and the TRANSACTIONS of the Society.

At present, the PROCEEDINGS publishes papers less than twelve pages long, and the TRANSACTIONS publishes papers longer than twelve pages. Professor Dyer views this as a pointless distinction, and he proposed abandoning the PROCEEDINGS and changing the format of the TRANSACTIONS. The new journal would be divided into a number of sections (possibly four), each dealing with a different, broad area of mathematics. The sections would be published separately. Each section would have an editorial board. When enough manuscript pages were received in a given mathematical area, an issue of the appropriate section would be published. The annual number of pages per section would depend only on the number of pages of manuscripts accepted. Individuals would subscribe by section. This procedure would reduce the time from submission to publication to less than six months.

Independently of these suggestions, Professor Dyer made a plea for retention of the refereeing system for all publicly communicated research results. He suggested that even research announcements be refereed, to eliminate graduate student-level material and to discover errors and duplication of results. He suggested that, unless a reputable mathematician asks for a referee, his papers could be published without refereeing until such time as he destroys his reputation through publication of bad papers.

As a device to insure careful attention to manuscripts by referees, Professor Dyer suggested that the following statement be printed on the first page of every paper published: "This paper was refereed and recommended for publication by_____." This would raise the overall quality of manuscripts published, but it might increase the problem of finding people willing to referee. Professor Dyer suggests that this problem might be solved by instituting a requirement that those who publish must also referee. Each person who published a paper would be expected to referee about two manuscripts.

The final speaker on this panel was Professor John Tukey, Princeton University. Professor Tukey noted that, at present, 90% of all mathematicians that ever lived are now alive. There have always been more mathematicians at a given time than when the mathematical elite of that time obtained their Ph. D.'s. One man can keep up with the work of only so many others. There are only three ways to face increasing numbers of mathematicians. One is to let only 50 people publish; this is not very acceptable. Second, the addressing of published items can be improved so that an individual has to search through less material to find items he wants. Third, every individual's scope of interest can continue to narrow as it has in the past. It seems apparent that, as once mathematicians gave up knowing about science, and later gave up knowing about mathematics as a whole, and as algebraists have given up or will soon have to give up knowing about algebra, so mathematicians are going to have to have some further narrowing of understanding.

Professor Tukey noted that in mathematics, the quality of publication has not yet gone down, and this is clearly a good thing. However, even if it had, deliberately restricting publication would not be easy to defend. It is too easy to say that we must get rid of trash as the solution to the growing flood of publication.

Publications serve many purposes, one of which is establishing intellectual priority. Tukey feels that, if a man asserts a result but does not provide the proof, the theorem should be known by the name of the man who provided the proof and not the man who asserted the result. Such a system would provide the leverage to get mathematicians to provide proofs.

In the future, the journal for the library and the journal for the individual are going to have to become two different things. The economics of both reproduction and delivery and of storage must play an important part in shaping both. Both library and individual journals may and probably should have a single title and a single editorship. However, their format and their purposes need not be anything alike. Professor Tukey said that printing in reduced sizes or producing publications in microfiche or ultra-microfiche may be the answer for libraries, at least for marginal journals in a given library. For some libraries, marginal journals might be all but five journals. He stated that he believes the distinction between the central and the marginal journal may well turn out to be one of the biggest changes which takes place.

The individual journal, on the other hand, will have to provide two or more qualitatively different services: first, selective dissemination of the full text; second, selective dissemination of twenty percent of the text in condensed versions; third, universal distribution of extended abstracts. If it were possible to provide extended abstracts of all papers in the TRANSACTIONS, plus detailed texts in the areas an individual would be interested in, it would be possible for a mathematician to read the TRANSACTIONS. The second of the three formats mentioned--condensed versions--may not be used, but it is clear that something between the extended abstract and the full text will be useful enough to be produced.

In addition, in order to obtain good surveys or review articles, it may be necessary to include them in the best primary journals. It may not be enough to create a journal for expository writing.

Photocopying is here to stay, and this method may be used for a substantial share of the selective dissemination of texts. It would be possible, by this means, to reduce the cost of centralized dissemination and publication of full texts, although centralized dissemination would probably have to be supplemented by local photocopying. A man will

obtain his information by both means, and if the central dissemination cannot be done more cheaply than local photocopying, the majority will be done locally. Methods of journal support must be adapted to take into account the fact of photocopying. Publishers have already begun to discuss the sale of books to libraries accompanied by a free right to photocopy for that library's clientele at a somewhat increased price. This plan could be adopted by the mathematical journals, or the journals themselves could be sold, with dissemination of selected items from them provided to individuals at a cost below the cost of retail copies. The page charge concept could be extended to provide further support for the preparation of those journals which allow free photocopying.

The individual badly needs a super journal also, one which reprints selected items, e.g., the full text of two to five percent of all papers and ten to twenty percent of all condensed versions, from some ten to twenty journals of importance or from a larger number of journals of mixed quality. Since the super journal would publish papers about twelve months after their original appearance, the contents would be used for general and overall information. The delay would protect the primary journals whose papers were reprinted.

The conventional journal would continue, but in a somewhat depreciated form. Professor Tukey indicated that he feels it is more realistic to approach the problems of publication from the point of view of introducing new forms rather than from that of improving present forms of publication. The essential solution is to introduce new levels of publication to get research papers imbedded in the archives and more or less immediately distributed to the people who are interested.

At noon, the conference participants attended the dedication of the new AMS headquarters building. Speeches were delivered by The Honorable John H. Chafee, Governor of Rhode Island; The Honorable Joseph A. Doorley, Jr., Mayor of Providence; and Dr. Jerome Weisner, M.I.T., Science Advisor to the late President John F. Kennedy.

After the dedication ceremony, Dr. William S. Barker, NSF Office of Science Information Service, and Mr. Ralph O'Dette, Chemical Abstracts Service, addressed the participants.

Dr. Barker stated that he hoped a system program or action plan for the development of a national information system in mathematics would result from the discussions at the conference. The NSF is prepared to assist mathematicians to create and set in operation new or improved capabilities for making mathematical information available, subject to budgetary constraints and priorities.

He stated that, to qualify for such assistance, an institution must satisfy two criteria: 1) It must have a mandate of the intended user community to operate and develop information services for that community, manifester in the charter or the by-laws or inferred from the fact of widespread supervision of current information services; 2) It must exhibit a posture of readiness, evidenced by a commitment to an accompanying system program or by progress toward definition of such a program, including items like formation of a system concept, achievement or pursuit of a planned base line, and build-up of a qualified staff and facilities. Commitment to one or more projects in the area is not enough evidence of readiness.

Why define the system program? The system program must be defined to provide an explicit framework for assigning priorities to objectives and for allocating resources to the task; to provide a road map; and to provide the basis for required funding. An explicitly defined system program will present alternative courses of action and costs.

How can the system program be defined? Given the existence of a generally accepted concept, three additional conditions must be substantially satisfied. First, a capability for the direction and management of the system program must exist within the responsible society or group. Second, the developmental base line must be known; that is, a description of the starting point, the existing system, must be developed. Finally, the deficiencies of the existing system must have been identified and analyzed. The deficiencies may be of two kinds: inadequate service to users, and inefficient operation.

The activities undertaken to achieve the prerequisite conditions are legitimate elements of the system program and as such are eligible for Foundation support.

What is the system program? It is a management tool consisting of a schedule of events and activities. Activities in the system program would include system engineering, applied research, system implementation, transitional operation test and evaluation, research oriented to the more advanced capabilities of future system generation. It is reasonable to expect the schedule to be backed by such exhibits as a market analysis and forecast of operating revenue and funding requirements.

As viewed by the NSF, the development of a system program for mathematics is a high priority item. Dr. Barker stated that he was hopeful that the mathematics community, as represented by the individuals and organizations participating in the conference, was prepared to see that such action is initiated.

Mr. Ralph O'Dette described Chemical Abstracts Service and the work it has accomplished in the area of constructing a computer-based information system.

CAS deals with the fields of chemistry and chemical engineering. Its objective is to make information in those fields available and used. In chemistry, the literature experienced a growth rate of about 11% in 1966 and about 14% in 1967. The service screens about 12,000 primary journals for abstractable papers in chemistry and chemical engineering. Since there are approximately 35,000 primary journals in the world, this means that chemical engineering information exists in about one third of the world's primary scientific literature. However, 75% of the abstracts published in CHEMICAL ABSTRACTS come from about 800 journals; to raise the percentage to 85%, another 1200 journals must be scanned. One year's output of CA will contain abstracts from about 7,000 sources. Only about 300 journals are actually chemical journals; that is, there are only about 300 journals from which CA abstracts all of the substantive papers.

The CAS staff has doubled since 1963. It now consists of about 1000 people full time. 60% of the abstracts published are provided by a volunteer staff of 3300 part-time abstractors scattered all around the world.

About 78% of the CA budget goes into production; 22% goes into research and development of improved methods for handling information. NSF has supported CAS programs since about 1958 or 1959, and the Foundation has been joined at various times by the National Institutes of Health, the National Library of Medicine, the Food and Drug Administration, and the National Cancer Institute. Besides the grant program, CAS is financially independent; that is, it does not receive any assistance from the member organizations or any membership dues itself.

CAS produces awareness tools, search tools, correlative tools, and custom services. Mr. O'Dette described CAS services: CHEMICAL ABSTRACTS, its indexes, and the list of periodicals abstracted in the journal; CHEMICAL-BIOLOGICAL ACTIVITIES and related tape and search services; CHEMICAL TITLES and related tape and search services; POLYMER SCIENCE AND TECHNOLOGY and related tape and search services;

a bibliography publication entitles STEROID CONJUGATES, and the RING INDEX.

The computer is used in the processing of CAS information because of the growth in the literature; the processing capabilities and reliability of computer hardware are essential to the processing of vast quantities of information. All intellectual analysis is done by people. CAS has had no success with automatic abstracting and indexing, machine translation, etc. Mr O'Dette feels that, at least for the foreseeable future, intellectual input will be prepared at the front end of the system by people. The computer will process the information and produce multiple output, recognizable as CHEMICAL TITLES, CHEMICAL ABSTRACTS, etc.

In 1968, CAS plans to put all headings of all of the abstracts onto magnetic tapes. Then, between 260 and 275 thousand abstracts will be in the computer, computer manipulable, and therefore available to be extracted in various information services.

In 1967, CAS also conducted a selective dissemination of information experiment which was successful, and an SDI service will be available in 1968.

Mr. O'Dette then touched on the question of interface. CAS has agreements with 65 other organizations, information services, university groups, companies, and government agencies for research projects and development projects. The joint operational program is very important and financially useful for the user. CAS is basically a chemical information organization, not simply an information organization, and it is dependent on the information handling techniques developed and contributed by other disciplines.

Mr. O'Dette stated that he feels the computer is finally going to provide a good objective medium for achieving cooperation among organizations. When several services can maintain their inventories in computer-manipulable machine language, it will be possible to share processing centers--information specialist centers, which are becoming an increasingly important part of the information scene. Some means for small systems and large systems to communicate directly with each other will have to be developed.

Mr. O'Dette then discussed the time lags between acquisition of information and its publication. The median delay time for the appearance of abstracts in CA is about fourteen weeks. It requires two weeks to obtain the source, eight weeks to obtain an abstract, get it edited and sent to the printer, and four weeks to print and bind the issue and send it to the post office. The computer-based publications require less time, and CHEMICAL TITLES and the polymer publication require four weeks only.

The lecture concluded with a discussion of what CAS has done in the area of development of a national information system. It is now conducting experiments where gross volumes of information are provided to information specialist centers which manipulate the information, mix it with information obtained from other sources, and provide information services to individual users. The goal of CAS is to be a sort of wholesaler of information, not a retailer.

The Panel on Communication with Other Disciplines convened at 2:30 p. m. on December 6. The Moderator, Professor Mark Kac, Rockefeller University, introduced the first speaker, Professor George Carrier, Harvard University.

Professor Carrier noted that there is very little communication between mathematicians and users of mathematics. He said that the small sub-fields which make up the population of users of mathematics have proliferated in the past few decades, and the worker in each of those sub-fields has trouble keeping track of everything that goes on in that area. For example, in the two exceedingly closely related fields of mechanics of solids and mechanics

of fluids, there is almost no professional communication, between the groups or between the individual members of the groups, despite the fact that the tools they use, the mathematics they call upon, are almost precisely the same. Their attitudes are the same, and their modes of description are almost the same. Why then should one expect to find any communication between two such different disciplines as mathematics and applied mathematics? Their skills and tools are different, and their objectives are almost entirely different. The mathematician wants to infer something of mathematical substance, and the user of mathematics almost invariably wants to make a scientific inference in the context in which the study originally arose, using the mathematics only technically and incidentally as needed.

The question is, Does it matter that there is no communication between pure and applied mathematicians? Professor Carrier believes that it does not matter, because existing pressures cause individuals to produce frantically in whatever narrow fields they can master, just to maintain their positions. Almost no one looks very far outside his own field, no matter what his opportunity to do so may be.

However, a few people have a large enough interest and find it valuable, if they are mathematicians, to discover problems that arise in science in the general sense. Their results, when based on substantive problems in science, can be exceedingly profitable.

How can we enhance the opportunities of the few who might communicate across the fence? Professor Carrier believes that the key lies in undergraduate education. Undergraduate mathematics students should not be allowed to graduate having no idea of the attitudes of users of mathematics, and students in applied mathematics should not graduate without some idea of the real objectives of pure mathematical studies.

The second speaker was Professor Jerome Lettvin, M.I.T. Professor Lettvin stated that he has noticed in the past two or three years an increasing tendency for pure mathematicians in the graduate school to investigate some aspects of the applied sciences, to want to look at the problems and to become involved in them.

For example, in his research on the nervous system and the messages the nerve fiber carries, Professor Lettvin found that the message a single nerve fiber carries represents a general point of view about nearly everything happening in the animal, and that this relationship can be handled statistically. Through problems such as these, pure mathematicians may be wooed into the applied sciences.

The final speaker was Professor Elliot Montroll, University of Rochester, who spoke about how the physics community is dealing with the communication problem.

He noted that about 5,000 pages per month are being published in American physics journals, and about 6,000 pages per month in foreign journals. While this looks like another hopeless literature problem, it is being dealt with fairly effectively.

About 60% of the material is more or less forgotten as journal publication within two or three years. This is because of the enormous amount of physical data that appears in the physical journals. This data gets absorbed in various tables, like the Nuclear Constant Tables that are published by the National Bureau of Standards. Therefore, unless one wants to see how an experiment was done three years ago, there is really no point in looking at the older experimental papers.

If a new Ph. D. is unsure of what field he wants to go into, Professor Montroll believes he can come up to date on almost any active field in physics in three to four months. He can do this because of the number of summer institutes he can attend. Recent contributors in an active area meet for three to six weeks at one of these institutes. The most important thing about these institutes is that the proceedings are published fairly quickly.

Then there are a number of annual reviews which list and describe current papers.

The next type of activity which is well-organized in physics is keeping up with what happened yesterday instead of with what happened last year. Publications which accomplish this are the PHYSICAL REVIEW, PHYSICS LETTERS, etc. Then there exist the invisible colleges which have on the order of ten to one hundred members that exchange preprints.

The PHYSICAL REVIEW LETTERS come out once a week. Not everyone reads his copy the day he receives it, but every physicist has three colleagues in offices near his; so each man reads one out of three papers and one of his neighbors will point out the papers he has missed which are pertinent to his field. Thus it is easy to keep up to date.

There are also collections of reprints of great papers. There are many more of these in Japan than there are in America, because after WWII few people in Japan could afford to subscribe to the good American and European journals, so they made collections of reprints of the best papers. All of the classic papers produced since the war can be put on one or two shelves, and this provides a good summary of the status of the field of physics.

Professor Montroll then discussed duplication of results. He stated that some individuals advocate automatic recovery of the literature as a solution to the problem of duplication. He said that he believes that duplication results more from familiarity with a printed paper than from unfamiliarity with its existence. When a man has a good idea and publishes his results hastily, several people see his paper and produce five similar papers clarifying the original results.

Duplication also results when several people see a printed suggestion for an experiment and conduct the experiment independently, producing and publishing similar results.

Another example of duplication occurs when physicists attempt to educate themselves in some area of mathematics which has applications to physics. They attempt to work their way through a particular problem, using all the material they can find in the classical literature, and several different papers which really correspond to self-education in that area are produced.

Professor Montroll suggested that it would be useful if detailed, descriptive books on a field or a new branch of mathematics were available so that physicists and other nonmathematicians could find out whether they are interested in a new field, or whether it has an application to their fields.

He proposed that mathematical books devote the first fifty or so pages to a description of the problem under discussion, where interest in that problem originated, etc. Or a series of about half a dozen books could be written which, in his opinion, could adequately describe the origin and development of all the main ideas in mathematics.

Following dinner on December 6, Dr. Robert W. Cairns, Committee on Scientific and Technical Communication, National Academy of Sciences, delivered an address. He was introduced by Professor A. Adrian Albert, Ex-President of the AMS.

Dr. Cairns described why SATCOM was established, in what matters it has interested itself, and how it hopes to fulfill its mission. He stated that recently the communication situation has become so diverse and complicated that a most pressing need has arisen for rationalizing the literature of science and technology, to make it more readily accessible and to organize it into more convenient packages for specific individual needs. This task was presented to SATCOM in the following words:

"The Committee will be expected to examine in broad perspective, and to make recommendations on, the present status and future requirements of the members of the scientific and engineering community with respect to structuring, flow and transfer of scientific and technical information and insight.

"Since information activities of the Federal government combine with those conducted in the private sector (by groups and organizations of all kinds; in the United States and, to a lesser degree, abroad) to serve all the individual scientists and engineers of the United States, and their organizations, and since the demands upon this combined effort are rapidly increasing, the committee will give special attention to:

> "1. information activities, policies and relationships, inter-relating the groups and organizations of the private sector (both at home and, as relevant, abroad), and
>
> "2. interactions and interrelationships between the Federal government and the private sector in connection with scientific and technical information, especially Federal executive or legislative actions or operations that affect substantial portions of the private sector,

with a view to making recommendations to the organizations and individuals of the private sector, as well as to the Federal government.

"The Committee will also consider, particularly (but not exclusively) in the light of the overall problem:

> "1. methods for promoting more effective relationships between information systems and the scientists and engineers, the principal producers and users of scientific and technical information;
>
> "2. techniques and systems for facilitating the effectiveness of information transfer; and
>
> "3. needs for new means of providing greater selectivity and consolidation in information transfer."

For the first year and a half, the SATCOM members reviewed the information activities of technical societies, private publishers and the major government agencies.

A classification of problems and issues into seven areas was finally made as follows, and seven groups were formed to deal with them:

1) Formal and informal primary communications. These would include verbal, written and printed primary communications of all kinds;

2) Secondary information services. This would include conventional abstracting and indexing activities of the technical societies and government agencies, and private alerting and retrieval services;

3) Libraries and library networks;

4) Copyright problems;

5) Standards and coherence of information systems;

6) Critical reviews and aggregation of information;

7) Advanced concepts in information transfer.

The objectives of this latter group are to seek practical paths to some of the way-out prognostications of the leaders of computer technology as to the integrated and automated systems potentialities now in sight.

Because of the immense importance of organization in respect to efficient scientific and technical communication, SATCOM's attention is focussed primarily on people and organizations of people--on scientists and technologists and the formal and informal groups that they have organized among themselves, and on the activities and policies of these groups.

Secondarily, SATCOM is concerned with the development and efficient utilization of particular information technology. It hopes to bring the current situation and future trends into clear enough focus to reveal what factors may inhibit the effective flow and transfer of scientific and technical information and what factors can be developed to facilitate such flow and transfer.

Dr. Cairns then described SATCOM's recommendations.

On meetings: Many technical societies have ended up with a pattern of meetings that may have a historical explanation but does not reflect careful analysis of usefulness on any grounds but popularity. Other criteria for evaluation should be found. Technical societies can take some positive steps to deal with proliferation and confusion, e. g. to structure meetings over a wide range of informal-formal patterns. Large meetings of wide geographical importance should provide standards of program review comparable to journals. Pre-meeting and post-meeting copies of papers presented may be provided. Critical discussion could have priority and be recorded. More attention should be paid to the interpretation of progress in active fields: 1) by organizing meetings to cover important areas in timely fashion; 2) by selection of reporters from "in-groups" to act both during and following meetings to facilitate dissemination.

On primary publications: Publication programs should diversify their media of formal communication, distinguishing those which serve primarily announcement purposes from those which provide the archival record. New editorial processes should be explored and instituted which couple the required effort more flexibly to the objectives to be attained, minimizing the work load and time delays. Possible means include paralleling the evaluation process with availability of papers prior to formal publications; requesting the submission of both brief and extensive versions of each manuscript, one of which is

selected for publication if the work is acceptable; and providing ground rules, together with the necessary bibliographical information, on widely distributed pre-publication versions.

The National Federation of Science Abstracting and Indexing Services should be sponsored in the development and promulgation of guidelines for authors and journal editors to use in adapting the form and format of primary literature to facilitate its processing in abstracting, indexing and related access structures. Experiments should be funded to provide information on the effectiveness and the costs of the major types of schemes for the distribution of items of information, "on request" as well as SDI.

On secondary publications: It is recommended that SATCOM and COSATI together with the NFSAIS promptly engage in serious discussions of mechanisms for more effective coordination of secondary services. It is also recommended that SATCOM help develop a plan for continued government support for non-governmental secondary services, and that these services be considered as part of a comprehensive system or network, with no attempt made to centralize them physically as in the case of the Russian agency, Viniti. It is also recommended that efforts be made to discover ways in which other governments could contribute to the furtherance of abstracting and indexing activities.

On libraries: A network of libraries should be encouraged to foster greater comprehensiveness of coverage and better communication. Libraries might consider charging for some of their services.

On standards and coherence: SATCOM should establish a working group to monitor the situation in the area of standards pertaining to information transmission and processing. The working group should have one member from each of the following sectors: government, universities, indexing and abstracting services, and the commercial information services. It is suggested that SATCOM recommend that government agencies provide long-term support to research on measurement of performance of systems for scientific and technical communication.

On reviews: Adequate incentives for qualified experts to write more and better reviews must be created. In each field of science, extensive studies should be carried out, with the active participation of experts in the field, to determine how much potentially useful information is currently being missed by workers in the field.

On advanced concepts: This is the newest area of SATCOM concern. It was decided that this group should focus its attention on the opportunities for accelerated information exchange made possible by modern computer technology. It should be unhampered by present-day structures in the information field or by rude questions of economics. This group will try to seek a moderate course between the extremes of instant information with unlimited expenditure and editorial deliberation at a modest cost.

After concluding his remarks on SATCOM's recommendations, Dr. Cairns closed by describing his own proposal for fulfilling the present need for more immediacy in primary communication.

He noted that the goals of the primary literature are prestige, perfection, priority, and promptness, of which the latter is least important. No respectable article reaches a position of esteem in the published literature without a lapse of a year or so in the reporting procedure. Therefore, the primary literature can only be characterized as archival. Being archival, the journal literature must of necessity portray lasting value. Permanence and quality are its goals, and editorial selectivity its method. Consequently, the deliberation of the process can be forgiven. However, other methods of communication must be sought

to take care of more immediate needs.

Dr. Cairns suggested that a system of distribution of informal communications could be the answer. A scientist could prepare an informal progress report in the form of a letter and transmit it to one of the distribution centers which serve the various disciplines. There it would be indexed in appropriate terms and its existence brought to the attention of the particular community which relates to that center. Through proper manipulation of the indices by machine handling, it would be possible to circulate author, title and key word lists in organized form quite broadly. The recipients could then request copies of the letters they wish to see, and through reprography the center would fill all requests. There would be no need for editorial screening, if the community would be willing to accept the letters as informal person-to-person communications between the authors and self-appointed recipients.

This scheme is immediate and conducive to machine transmission. The only intellectual effort required would be in the indexing, and this could be simplified by requiring authors to help in titling and selecting key words. Because the letters would be informal, they would also be temporary. A letter could be kept up to one year at a center, after which it would be returned to the author with a list of recipients, should he wish to continue the correspondence. Limited circulation of the letters need not be interpreted as prior publication, in the event that they were resubmitted as manuscripts for journal articles, and they would not be used to establish priority in a field. The scheme would also offer possibilities for anyone to participate in "in-groups" of his own choice and invisible colleges of his own design.

On the morning of December 7, Dr. K. Chandrasekharan, Secretary of the International Council of Scientific Unions, discussed some innovations which might be attempted in information exchange, particularly in the area of abstracting and indexing of the literature, on an international level.

He described the founding of MATHEMATICAL REVIEWS, JAHRBUCH UBER DIE FORTSCHRITTE DER MATHEMATIK, ZENTRALBLATT FUR MATHEMATIK, and REFERATIVNYI ZURNAL, and the importance of these journals, particularly of MATHEMATICAL REVIEWS, to mathematicians in underdeveloped countries. He suggested that more cooperation among the world's producers of mathematical reviewing journals should be fostered, and suggested a type of management where control would rest with an international committee and where production and distribution would be handled commercially.

He noted that the question has been raised whether it might be cheaper to have one reviewing journal translated into several languages, rather than several journals as sources. Cost would be an important factor here.

He also suggested that reviews might be produced by referees in order to decrease the time lag between the publication of a paper and the appearance of a review. In 1966 the General Assembly of the International Mathematical Union recommended that the reviewing journals request referees to encourage a policy of cooperation between primary journals and reviewing journals.

Another possibility would be the publication of refereed author abstracts.

Dr. Chandrasekharan stated that it would be desirable to have,separate from any reviewing journal, an index of the world's published mathematical literature. The problem of classification of titles could be solved by cooperation among existing reviewing journals.

Such a publication would be relatively inexpensive and could be computerized.

The last panel of the conference, the Panel on Reviewing Journals, met at 9:30 a.m. on December 7. The moderator, Professor W. J. LeVeque, University of Michigan, introduced the first speaker, Professor Irving Segal, M.I.T. Professor Segal advocated discontinuing publication of MATHEMATICAL REVIEWS and instead publishing an abstract journal similar to PHYSICS ABSTRACTS.

He stated that, historically, the REVIEWS came into existence because of the failure of ZENTRALBLATT to uphold elementary standards of scientific objectivity. The availability of well-qualified editors at that time, together with the small volume of scientific publication in the early 1940's, made possible a journal of the highest quality. However, Segal said that conditions changed with the end of the war, and it has been progressively more difficult to maintain the average scientific quality of the operational staff.

Administratively, the REVIEWS has had the serious problem in the past two decades of a vast increase both in the volume of publication and in the rate of scientific change. Many reviews appear long after the original date of publication. However, even the best possible administration seems likely to leave a substantial interregnum between the appearance of a paper and of its review, precisely the period during which the review should be most useful from a scientific point of view.

Professor Segal went on to say that, institutionally, the REVIEWS forms a large-scale organization and, over a period of years, such an organization tends to acquire vested interests and unconsciously to identify its organizational welfare with that of mathematics in general. This feeling is not necessarily justified.

He then discussed alternatives to MR, stating that the question is, What is the very best use which can be made of the scientific manpower which is presently utilized by MATHEMATICAL REVIEWS? He is convinced that a journal similar to PHYSICS ABSTRACTS would be relatively more useful. The cost in terms of scientific manpower would be negligible in contrast to the very considerable cost of the present REVIEWS, and, on the whole, it would be scientifically equally useful. The relatively short period between the publication of articles and the appearance of their abstracts would greatly facilitate investigations in currently active fields. Also, such an abstract journal would free the scientific manpower to staff an expository journal on the order of USPEHI, which Professor Segal feels would be a more useful application of the talent in question.

The second speaker was Dr. Stephen Juhasz, APPLIED MECHANICS REVIEWS. He outlined the differing scope and purposes of a critical reviewing journal and an abstract journal. An abstract journal can supply the reader in a relatively short time with the information bringing to the readers' attention the availability of the paper and its content. A critical abstract journal in addition evaluates the paper, indicates the optimum readership and also performs a filtering activity by omitting the mention of those publications where the reviewer indicates that the value or novelty of the paper is such that it should not be included. Obviously, this activity takes longer time and a critical review will be published a few months later than an indicative or informative abstract. The two services can compliment each other and it is also conceivable that an abstract journal indicates (adequate coordination is needed) in the annual index the availability of the abstract and the critical review by giving the suitable references to both services.

Dr. Juhasz indicated the difficulties in handling, with a computer, mathematical titles. This seems to be a paradox as computers were developed by mathematicians. The KWIC type indexes are based on the significant words in the titles and their number is much smaller in mathematical papers than in titles of any other discipline. This difficulty

could be overcome by having, in addition to the usual brief titles, also some sub-titles. The question of computer handling of formulas given in the title was discussed and suggested that they be given in Fortran language.

The uniform treatment by computers of bibliographical citations and adaptability for computer handling requires certainly a standardized bibliographical citation.

Dr. Juhasz made a slide presentation showing the computer-made WADEX (Word Author inDEX) System developed at AMR and the University of Texas, showing a sequence of slides where on each slide a new element is added to the information shown on the previous slide. The last slide showed the entire system as printed by the computer.

The third speaker, Professor Ralph Boas, also discussed the merits of reviews versus author abstracts.

He said that author abstracts have two obvious advantages: They can be published faster than reviews, and the use of author abstracts would eliminate the occasional bad review produced by obtuse, malicious, or overly enthusiastic reviewers. Using author abstracts would also save time for the mathematical community. However, the amount of research lost because of reviewing time must be negligible. When MR was founded, one distinguished American mathematician feared that the effort expended in reviewing would seriously impede the progress of research in this country. Of course he was wrong, and the progress of research has been greatly helped by the existence of MR. Professor Boas recalled that the young mathematicians of 1939 felt flattered when they were asked to be reviewers, and took their reviewing very seriously. He noted that perhaps the young mathematicians of today would respond similarly if asked; there are about ten times as many of them as there were in 1939, but only about seven times as many papers to be reviewed.

He went on to say that there are, of course, people who don't believe in reviews because they subscribe to the theory (which he does not believe) that the only mathematics that counts is done by great men at great centers, who don't need anything but personal contact with the other great men. The rest of the ordinary mathematicians, however, depend on MR for information about what is going on, and reviews are more useful than author abstracts for a number of reasons.

The advantages of the reviewing system show up best for a relatively small proportion of papers, but since one cannot predict which papers these will be, it is best to treat all papers the same way. The reviews that really count are those in which the reviewer points out a missed reference, finds a mistake, or places the paper in its proper context. (It is fallacious to suppose that an author necessarily really knows what is in his paper.) Sometimes a reviewer even shows that a paper is more interesting than the author realized.

Professor Boas noted that authors do not always produce useful abstracts; if author abstracts are to be really helpful, they will have to be refereed and edited (at a considerable cost in time). One extreme is illustrated by a paper in Georgian that he once had to cope with. There as a Russian summary, and when he puzzled it out, it said "A translation of this paper into Russian will appear later." At the other extreme is the author who tries to pack his entire paper into the abstract; a reviewer can often save considerable space merely by pruning the verbiage, or deleting definitions of things that are never referred to again. Such authors apparently share the not uncommon idea that the length of a review should be proportional to the importance of the paper. Professor Boas stated that this is quite wrong. For an important paper, all that is necessary is to call attention to its

existence, since everyone will read the whole paper anyway. For a trivial or wrong paper, the review should be long enough to make it unnecessary to consult the paper at all. Indeed, one of the most important services of MR is to tell people which papers they don't have to read.

Professor Boas suggested that perhaps a synthesis of the two approaches (abstract versus review) would be worth considering. If an effort could be made to have authors supply abstracts, reviewers could, whenever possible, simply edit the abstracts, saving time for the editors of the reviewing journals and for themselves.

Dr. R. J. Crittenden, MATHEMATICAL REVIEWS, made some general remarks on reviewing journals. He stated that the willingness of over 2,000 mathematicians to review for MATH REVIEWS indicates at least an implicit approval of the present goals of the journal. In any decision to make major changes in a reviewing journal, the possible irreversibility of the process should be considered.

Publication of an abstract journal would probably involve at least three times the professional staff necessary for a reviewing journal, if the abstracts were not prepared at source. If a journal of author abstracts were to be considered, either 1000 journals throughout the world would have to be induced to require author abstracts, or abstracts would have to be solicited from the authors themselves. Dr. Crittenden noted that 5% of the present MATH REVIEWS consists of reviews of books; what would be done about these?

Dr. Crittenden went on to describe the future development and uses of MATHEMATICAL REVIEWS. He stated that, hopefully, the subject index would reappear in the near future. The compilation by Norman Steenrod of reviews in algebraic and differential topology has obvious potential as a research tool, and other such compilations might be attempted. Mechanization will clearly play a role in the future of MR, but total automation of the printing seems to be between five and ten years in the future. At the production end, mechanization in the near future will be limited to production of a five-year cumulative author index from punched tape, produced from the headings in corrected page proof. The step to production of subject indexes may be taken shortly; the principal obstacle seems to be in the handling of corrections--35% of all headings are corrected.

Concerning the problems of increased volume of publication and the time lag in the appearance of reviews, the first can be handled for a number of years by existing procedures. At best, a reduction of the minimum time lag from eight to six months can be achieved, along with a reduction in the volume of material each reviewer must handle.

The fifth speaker was Frau Erika Pannwitz, ZENTRALBLATT FUR MATHEMATIK. She discussed the history of the world's reviewing journals.

The oldest mathematical reviewing journal, JAHRBUCH UBER DIE FORTSCHRITTE DER MATHEMATIK, was based on principles different from those of present reviewing journals. Reviews for the literature of one year were gradually assembled, arranged systematically in several sections, and published in one volume appearing three or four years later. There was an author index and a list of journals reviewed, but a subject index was not needed because of the systematic arrangement. The most significant shortcoming of the FORTSCHRITTE, apart from the delay in publication, was that the system simply was not flexible enough to keep up with changes in the structure of mathematics.

ZENTRALBLATT was founded in 1931 in order to meet the desire of mathematicians for prompt information. It provided author, subject, and cumulative five-year indexes, but it was less suitable for long-term information than the FORTSCHRITTE. The two journals existed side by side, each fulfilling its own task.

During the Second World War, MATHEMATICAL REVIEWS was founded in the United States and, in Germany, the offices of the FORTSCHRITTE and ZENTRALBLATT were placed under one editor-in-chief. To lessen the burden of reviewers, the editor-in-chief had the right, for less important papers, to publish the same review in both journals. It was a challenging experiment to see whether the two reviewing journals, one for prompt information and one for long-term information, could be carried on jointly with a reasonable burden on reviewers. However, the disruption in Germany at the end of the war halted the experiment; in 1948 ZENTRALBLATT resumed publication, but the FORTSCHRITTE never revived. Then, in 1953, the Soviet Union instituted REFERATIVNYI ZURNAL.

At the present time, the growing bulk of mathematical publication and the increase of other difficulties in reviewing makes it imperative that the three journals begin to cooperate in every possible way. How can they cooperate? The ideal solution might be a union of all three, but such a solution is not realistic in the near future.

Frau Pannwitz suggested that the three journals unite to produce cumulative author indexes in a form containing material from all three. She also suggested that the three journals produce combined cumulative subject indexes. She noted that a joint effort of this kind could result in still further cooperation.

The final speaker was Professor Kurt Jörgens, University of Heidelberg. He disclosed the results of recent discussions of the advisory board of ZENTRALBLATT. The board has concluded that it is unreasonable that both MATHEMATICAL REVIEWS and ZENTRALBLATT should continue to publish reviews of the same type, sometimes in the same language, and quite often written by the same reviewer. The board feels that two kinds of information--reviews and abstracts--are needed by the mathematical community. Accordingly, ZENTRALBLATT has decided to begin immediately to publish authors' abstracts in place of reviews where possible, with the intention of concentrating exclusively on the publication of such abstracts as soon as possible. It is presently the intention of the journal to request the individual authors to provide abstracts. Jörgens stated that he hopes that the editors of primary journals will cooperate with ZENTRALBLATT by requesting abstracts to be provided by their authors together with their papers, requiring very little further processing in the ZENTRALBLATT office.